

Concise Instructions in the Art of Retouching. By Burrows and Colton. (London: Marion and Co, 22 and 23, Soho Square, 1876.)

FOR some years past a conviction has been growing amongst the better class of professional photographers that their art alone, even under the most perfect conditions, is unable to produce an artistically perfect portrait, a proposition, by the way, for which the true artist has all along contended in the face of the constant assertion of the converse by fanatical advocates of "sun-painting" *pur et simple*. The victory having at last rested with the artists, a number of books on retouching have been published, each professing to give the true method of at once producing artistic pictures.

We are glad to see that in the present little work the authors disclaim any such intention, but, on the contrary, proceed solely to instruct their pupils in the work before them, which we may here inform the uninitiated is no less a one than that of restoring, so to speak, on a photographic negative those injuries to a face which may have been caused by imperfect lighting and defects, such as dust, &c., in the film; or disease or physical injury to the face itself. In fact, the art of the re-toucher is to convey to a photograph a certain amount of that idealisation always manifest in the works of the painter, the want of which is the unknown cause so often producing a feeling of dissatisfaction even with the best of photographs.

To carry out their aim, the authors give two very good lithographs of the muscles of the face and head, with two more of the same model covered with the flesh. Two negatives on a flexible film (apparently taken by Warnerke's process) are also added as examples of the work to be done. The descriptive matter is concisely put, and is clear and to the point. We have little doubt that the book will be of service to many amateur and professional portrait photographers.

R. J. F.

LETTERS TO THE EDITOR

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Sea Fisheries

I HAD hoped that Mr. Holdsworth, in the rejoinder which he told me he was preparing to my former letter (*NATURE*, vol. xv. p. 55) would have confined himself to defending the assertions he had before made, or at most to rebutting the evidence I had adduced in reply to them. In this case I should have gladly left the matters at issue between him and me to the judgment of the public. Unfortunately he has thought it needful for the sake of the cause he adopts to introduce some new assumptions and charges, conveyed in language of a rather vigorous kind, so that out of regard to the good opinion of your readers, I am driven again to trespass on their forbearance and yours. But in doing this I shall try to be as brief as possible, and however much my friend may have exceeded the limits of a rejoinder, not to follow his example.

All will fully agree with Mr. Holdsworth that an "index is not a *précis*," but few will deny that an index is a valuable aid to mastering the contents of a book. If he says that this particular index is a bad one, I must leave him to settle with the maker of it. If it is neither bad nor good it may be misleading unless the user of it looks pretty carefully into the text. But if it is good, as I believe, it gives the reader the best of all help in acquainting himself with the huge volume, and by its help nobody need fear falling into dangerous mistakes. It does not seem to me that I have fallen into such. The errors which my friend asserts I have made are, if errors at all, very trivial, and as one tells for, and the other against, his views, they may be safely paired off to the detriment of neither side. As to the figures set in the last column of my table, against "Cod and Ling," they ought to have been "33" instead of "38"—a mistake in copying or printing which escaped my observation till now. I freely give Mr. Holdsworth the benefit of it. The next two paragraphs of his letter have afforded me some merriment, though chastened by

the thought that he must have a very low opinion of me if he seriously supposes I am ignorant of the notorious reputation of the dog-fish. Whether "predatory fishes," however, are necessarily "mischievous," so that the two epithets should be closely linked together, as though one was the consequence of the other, is a large question, upon which I shall not enter. But surely it is obvious that the prevalence of predatory fishes is more or less a measure of the prevalence of their prey, and as the blind man judged of the value of the field by asking how many thistles grew on it, so may we judge of the abundance or scarcity of other fishes by the abundance or scarcity of the dog-fish. Might I here apply to Mr. Holdsworth an expression of his own to myself, and say that, from these paragraphs, I am justified in believing him unable to comprehend one of the simplest relations of animal life?

As to herrings, I pretend to no greater knowledge of their natural history than other people do. I do not see why I should be accounted more ignorant, or attempting to conceal that ignorance, by any mysterious evolutionary process from my inner consciousness or elsewhere. The herring is admittedly not ubiquitous in the sea, *i.e.*, it has, like other animals, its more or less definite range. It therefore has "borders," though even Mr. Holdsworth cannot lay them down exactly. My friend is pleased "to doubt very much" whether I "had given five minutes' attention to the practical study of the habits of the herring—to its life-history" before I wrote my Glasgow address. That rather depends on what may be called "practical study." Has Mr. Holdsworth ever heard of a "water-telescope"—an instrument of which I can find no mention in his book—or has he ever looked through one? If, when the days get a little longer and the steamers are running conveniently, he will cross to Norway and follow the coast to the Lofoden Islands (perhaps he need not even go so far), he will possibly appreciate the value of these remarks, and will be doing what I did more than twenty years ago.

The next five paragraphs of Mr. Holdsworth's rejoinder seem to contain very fair comments on what I had urged, and, though I do not thereby assent to them, I may say that had the rest been of apiece with them I should not now be troubling you. I must, however, express my disappointment that in what follows no definite information is given as to the sea-fishes which are so often said to be devoured by sea-birds. Here is room for almost any amount of new and interesting observations, whether those observations affect his argument or not. He, not I, introduced the topic, for reasons I suppose known to himself, but not to be guessed at by me. He now seems to consider it, as I did, irrelevant.

Then as to Prof. Baird's reports. Far be it from me to find fault with my friend for fishing out the two passages which, as he thinks, tell in his favour. But these relate to two particular kinds of fishes¹—the alewife and the cod—the former mainly as furnishing food to the latter, and I never said that to over-fishing *only* was the diminution in every case due. The decrease of the cod is ascribed by Prof. Baird to the decrease of the alewife, and this, he says, is caused by the erection in the tidal rivers of impassable dams or of weirs by means of which every fish ascending the river to spawn was caught. Surely this was "over-fishing." In the first of his reports this question is considered far more generally and closely than in the second, from which Mr. Holdsworth's extracts are taken, yet there is nothing in the latter really to contradict the conclusions arrived at in the former. Hence I infer that they are still upheld by their author, and their nature may be seen by the following citations from his "General Summary of Results" (Part I., pp. 38, 39):—

"I. The alleged decrease in the number of food-fishes in these waters within the last few years has been fully substantiated.

"II. The shore-fishes have been decreasing during the past twenty years, gradually at first, but much more abruptly from about the year 1865, the reduction by the year 1871 being so great as entirely to prevent any successful summer-fishing with the hook and line, and leaving to the traps and pounds the burden of supplying the markets. This statement applies also, but perhaps to a certain extent, to the blue-fish. The decrease in their numbers first manifested itself about ten years ago, and is going on quite rapidly until now.

"III. This period of decrease represents the time during which the traps and pounds have been well established, their operations

¹ The menhaden and the mackarel are indeed mentioned, but incidentally and with some uncertainty.

increasing year by year, and their catch, especially in the early spring, being always very great.

"VI. The decrease of the fish may be considered as due to the combined action of the fish-pounds or weirs, and the blue-fish, the former destroying a very large percentage of the spawning fish before they have deposited their eggs, and the latter devouring immense numbers of young fish after they have passed the ordinary perils of immaturity."

As Prof. Baird goes on to remark that there are no measures at command for destroying the blue-fish, even if that were desirable, and as the blue-fish was once far more abundant than it is at present, while other fishes were also more numerous, I cannot see that I made any mistake in stating that "over-fishing" was unquestionably assigned as "the chief cause" of the decrease in American sea-fisheries.

Lastly, Mr. Holdsworth says that the question lies between the late Royal Commissioners and myself. It was under this belief, holding him as their secretary to be their mouthpiece, that I took some trouble to reply to his first letter. Had any one not in that position challenged my remarks I should, perhaps, have not felt myself bound to give my reasons for the faith that is in me. He asserts that I have "no practical acquaintance with the subject." Possibly he considers that qualification limited to those who have been named in a Fishery Commission. In such case I certainly have none. He further charges me with using the Index to the Evidence as my "sole guide." Here I must venture to contradict him. I have used that Index, indeed, but much as Norwegian fishermen use the "water-telescope"—to look into the teeming depths of evidence below, unobstructed by the surface ripple of a Report.

To sum up. Your readers are aware that I originally treated of the Fisheries question as part of a much wider subject on which I felt constrained to speak my mind at a fitting opportunity. I have yet to learn that the Report of a Royal Commission is beyond the reach of fair and cool criticism, or that it is obligatory on all men to accept that Report as a revelation from supreme intelligence. My criticism of this Report was, I venture to think, not unfair, and it was not made in hasty warmth. Some ten years had passed since I adopted the opinions I hold, and the time had come when, as I thought, I could not help uttering them, nor does it seem to me that an unfitting occasion was offered by a meeting of the British Association. The decision of the question whether there is and has been "over-fishing" or not is hardly helped by the reiteration of the passage with which my friend ends his rejoinder.

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ALFRED NEWTON

December 15, 1876

Ocean Currents

AGREING in the main with Mr. Digby Murray's argument on the subject of ocean currents in NATURE (vol. xv. p. 76), I am the more disposed to criticise some of the statements with which it concludes, as put forward too strongly, to say the least.

I would ask for the "absolute proof" which Mr. Digby Murray supposes to exist, that (1) the upper-current return-trades "flowing from the equator descend again to the surface of the ocean on the polar sides of the calms of Cancer and Capricorn," and (2) "that these equatorial currents, subsequent to their descent on the polar sides of the calms of Cancer and Capricorn, are known as the westerly winds of the temperate zones." That these statements represent the prevailing opinion on the subject I readily admit, but I have ever looked in vain for any convincing arguments in their favour.

As regards the hypothesis that the trades cross one another in the region of equatorial calms, I may perhaps be permitted to quote some remarks of my own, made two years ago (*Symons' Met. Mag.*, vol. x. p. 37), since subsequent study has tended to confirm the doubts which I then expressed:—

"Maury's hypothesis, that the surface trade-wind of one hemisphere becomes the upper-current return-trade of the other ('Physical Geography of the Sea,' sec. 122 to 139) was in all probability originally suggested by the well-known fact that over the southern portion of the N.E. trade a S.E. upper-current prevails, and over the northern portion of the S.E. trade a N.E. upper-current, though he lays most stress on the arguments which he draws from the greater rainfall of the northern hemisphere (sec. 169 to 186), and from Ehrenberg's examination of the African air-dust (sec. 266 to 296).

"A seaman on approaching the doldrums, commonly notices a current overhead blowing at an angle of about 90° with the surface-trade; he is aware that this upper-current coincides in direction with the trade on the other side of the doldrums, and that in the calm belt itself, there is an upward motion of the atmosphere. It is, therefore, not unnatural that he should conclude that the upper-current which he observes is a poleward extension of the opposite trade in the higher regions of the atmosphere. It may also, I think, be admitted that the rapid and suddenly shifting cloud-currents, often observed over the region of the doldrums, are somewhat in keeping with Maury's idea of 'curdles,' or alternate strips of air.

"I would suggest that this hypothesis (which many subsequent writers have been surprisingly ready to adopt) may, perhaps, be subjected to a crucial test, if an answer can be given to the following query:—When the south-east trade draws so far to the north as to be deflected into a south-west surface wind, what is the prevalent direction of the upper-current over the southern portion of the north-east trade? If it runs from south-west it will be difficult to resist the conclusion that Maury is right; if from south-east it will appear probable that the upper-current is (principally at least) the north-east trade, deflected in the first part of its return course towards the north-west, just as it is in the subsequent part towards the north-east.

"Perhaps some meteorologist can give a definite answer to this question. The published data for its solution appear rather scanty; but, so far as my own limited information goes, the observations are generally rather adverse to Maury's theory."

I would now ask what proof exists that the upper currents from the polar depressions and those from the equatorial depression cross one another in the calms of Cancer and Capricorn so as subsequently to become the trades and anti-trades respectively? Since these upper-currents are understood to meet at the belts of tropical calms and there to descend, it is surely "more reasonable to suppose that their currents intermingle and that their mixed volume is then drawn off north and south, as required to restore the equilibrium of the atmosphere." These are Mr. Digby Murray's words in reference to the equatorial calms, and I fail to see why they will not apply to the calms of Cancer and Capricorn.

The whole question of the cause of the prevailing south-west and north-west winds of the north and south temperate zones, and the relation which these bear to the polar areas of barometric depression, may be regarded as fairly solved by the researches of Mr. Ferrel, Prof. J. Thomson, and others. As regards the great intensity of the Antarctic, as compared with the Arctic depression, and the superior force of the westerlies on its border, there is surely *prima facie* ground for believing that these are mainly due to superior evaporation in the water-hemisphere generally. (I say "mainly," because it seems probable that the comparative absence of surface-friction experienced by the atmospheric currents in that hemisphere tends to intensify the Antarctic depression.) That the evaporation from the warm surface-water of the North Atlantic is in excess of that from the relatively cold surface-water of the South Atlantic, may be readily admitted; but the Atlantic represents, after all, only a small portion of the surface of the globe. Will anyone maintain that the evaporation from the whole continent of Asia is equal to that which takes place from the corresponding area of the South Indian Ocean?

W. CLEMENT LEY

Solar Physics at the Present Time

HAVING now read the *Astronomical Register's* more extended account of the November meeting of the Royal Astronomical Society, I found it very confirmatory of NATURE's shorter, but more quickly produced, summary of November 23, especially in what was said in the discussion upon Prof. Langley's (United States) paper on Sun-spots and Terrestrial Climate. Will you kindly allow me to remark:—

1. I am extremely glad that Sir G. B. Airy is now finding from the deep-soil thermometer observations at Greenwich that, whatever may be the interior temperature of the earth, and the terrific manifestations of it in some special volcanic localities abroad, yet all the remarkable changes and occasional abnormal elevations of temperature in the Greenwich soil *come from without*; for, Sir, that is precisely one of the earliest conclusions which I deduced for the Edinburgh soil, from the longer series of similar deep-soil thermometers there, and which I had the honour